FEDERAL SECURITY AGENCY
PUBLIC HEALTH SERVICE

IN REPLYING, ADDRESS THE
USPHS Tbc. Research Lab.
411 East 69th Street
New York 21, N. Y.

December 2, 1952

Dr. Joshua Lederberg
Dept. of Genetics
University of Wisconsin
Madison 6, Wisc.

Dear Joshua,

Since it has not been possible for me to find time to spend in your lab., and since the scoring for resistance to PAB and POB analogues is tricky, it looks as though we had better try to do the heterozygote experiments here. Norton's presence in the neighborhood furnishes a good deal of moral support and he has already provided much helpful advice.

We have developed various kinds of resistance in your strain W 677 (TLT⁻, sugars neg.). For a cross with a het strain I presume we would want W-478 (BM⁻, sugars pos.). I also wonder whether we wouldn't improve the efficiency of our selection of diploid recombinants by using this or some other complementary strain which was lac_4^-, screening for lac^+ crosses with W 677, which I believe is lac_1^-. Finally, since it would be desirable to score both the parents as well as their recombinant for drug resistance, it would be desirable to use, as complementary to W 677, a strain that has some requirement other than methionine, serine, or purine. These compounds exert a marked effect on the inhibitory levels of the drugs we're interested in. Finally, if you had a complementary that had requirements other than these, and was also lac_4^-, so much the better.

It is very good to have Norton in the neighborhood; he has already stirred into action the idea, which has been hanging around for a long time, of having a journal club for the Rockefeller Institute plus our lab. on microbial genetics and related topics. Incidentally, I'd like to congratulate you as well as him on the transduction paper, from a literary as well as a scientific point of view. I have been wanting to talk to you for a long time about possible ways and means of making your papers easier for most of us to read, but it looks as though this problem has been solved. My only major criticism is that the biochemical aspects of the tryptophan-tyrosine-phenylalanine case are critical and should have been presented in a little more detail. The reference you gave to one of my papers really doesn't support your interpretation adequately, since the scheme presented in that paper doesn't make the matter clear unless one also goes into the business of incomplete blocks. A reference to aromatic paper IV, which appeared in

the same issue, would of course have covered the matter — but you didn't know, of course, that it was in press

I hope it won't be any trouble for you to get the het-producing strains to us soon, as Dr. Yaniv, a Fellow from Israel, is anxious to get started on this problem soon. Thanks in advance for the material.

Sincerely,

Bernard D. Davis

BDD/em